turbance propagated from one body is converted into an attractive force upon the other. And yet the present theories of electricity and magnetism are in the same state. It is simply known that they are the result of molecular waves, but the nature of the transformation is as yet a mystery. Clerk-Maxwell has given in six papers in the *Philosophical Magazine* for 1861-1862 a provisional theory for magnetism; but there has been no great advance made in this direction. That the full connection will ere long be discovered, is almost certain; and in the meantime it will not be without its purpose to point out that in the course of time it will, in all probability, be necessary to extend the same investigation to gravitation.

Art. XIII.—*Experiments on the Comparative Power of some Disinfectants.*

By James Jamieson, M.D.

[Read on the 11th October, 1877.]

The object of the present communication is to record the results of a series of experiments on the comparative power of certain disinfectants when applied in the form of vapour. While this department of the subject has undoubtedly great practical importance in many ways, it has been comparatively little cultivated, due no doubt, in some measure at least, to the difficulty which attends any attempt to carry out such investigations in an exact way. It so happens, therefore, that our knowledge on the subject of aërial disinfection is made up mainly of vague impressions, which may perhaps be tolerably correct, but which are greatly in need of a basis of well-established facts and scientific investigations.

It would be out of place for me to enter at any length on the general question of the nature of those remarkable processes included under the terms putrefaction and fermentation; but it is necessary to state the opinion I hold on the subject, which is that now generally accepted by men of science. It may be said, then, that putrefaction, fermentation, and other allied processes are in their essential nature chemical changes brought about in organic matters by the functional activity of minute vegetable organisms; these changes being of a destructive character, consisting in the reduction of complex substances into simpler ones. Certain phenomena which specially obtrude themselves on our notice, such as the formation of disagreeably smelling matters in putrefaction, and the copious evolution of gas in ordinary alcoholic fermentation, are mere accidents. Among the allied processes referred to must be ranked, I think, the changes going on in the animal economy in the course of certain acute diseases, which, from their apparent analogy with the phenomena of fermentation, have been long named zymotic. The investigations of some of the best pathologists of our own day have supplied evidence of a positive kind in favour of that theory; and with reference to a few of the acute contagious diseases there is, I think, satisfactory proof that they owe their origin to microscopic organisms belonging to the lowest order of plants. The doctrine of the parasitic nature of the ordinary epidemic diseases, founded partly on the analogy already mentioned, and more recently on the results of exact observation and experiment, has received a further confirmation from the beneficial results following the use of well-known disinfectants having a parisiticidal action in the cure, and still more in the prevention, of some diseases of the kind now under consideration. To prove the action of disinfectants in preventing or checking putrefaction in substances liable to it is easy; but when we have to deal with the living animal the matter becomes much more complicated, and hence perhaps the want of demonstrative force in the evidence adduced in favour of the action of disinfectants as preventive and curative agents in disease. An important step has been recently made by subjecting the virus or contagious matter of some diseases, such as glanders and vaccinia, to the action of disinfecting agents, and then testing its power of communicating the disease by inoculation. Such investigations have been carried on by Dr. Dougall, of Glasgow, and in a more thorough way by Dr. Baxter, whose experiments are fully described in the Reports of the Medical Officer of the Privy Council for the year 1875. It is there clearly shown that the ordinary disinfectants—carbolic acid, sulphurous acid, and chlorine—destroy the contagious property of the vaccine and glanders viruses when applied to them in the same manner and in the same strength as is found sufficient to destroy the organisms causing putrefaction, and thus to put a check to that process. The chain of evidence, therefore, seems very complete in favour of these two points—(1) that certain acute contagious diseases are caused by the introduction into, and multiplication in, the animal body of minute vegetable organisms; and (2) that it is possible to destroy the contagious power of the virus by means of disinfecting agents, and so prevent the spread of these diseases. There may be room for difference of opinion as to what diseases can be included in this class; but there has been almost absolute demonstration supplied of the correctness of one or both of these points with regard to certain, and among these are to be reckoned especially anthrax, glanders, remittent fever, diphtheria, and vaccinia. When the virus has taken root in the body, it is very questionable if we can do anything to stay its progress. This is owing to the fact that we cannot introduce these parasiticidal agents into the animal system, in amount sufficient to destroy the morbific organisms without at the same time doing irreparable injury to the delicate structures of which it is built up. But whilst we have thus to confess our impotence in the present state of our knowledge, and with the agents now at our disposal, I for one cherish the hope that the chemist, by means of the synthetical method of forming new compounds, will yet offer us some agent capable of doing all that is required. That salicylic acid has not done more to supply the want must have been to many, as it was to me, a grievous disappointment.

We are thrown back therefore on prevention as the great field of our activity in this department of practical medicine; and there we may with confidence look forward to triumphs greater far than have been already attained, considerable as these are.

As epidemic diseases generally spread by means of some virus, which has been formed in the body of animals suffering from them, and is conveyed in some way from these diseased animals to healthy ones, it is clear that if we could destroy with certainty all contagious matters as they leave the body the work of prevention would be done. That it is possible to destroy the viruses of all contagious diseases by mixing them with a sufficient amount of some disinfectant may be regarded as almost certain, since it has been actually done in the case of several of them. Unfortunately, we cannot always obtain the virus in substance, so as to operate upon it in that way; and we are compelled, therefore, to consider the possibility of attacking it when suspended in the atmosphere, or attached to walls or other surfaces in a dried state. That some diseases are conveyed by means of a dried contagium floating in the air seems to be certain, and therefore in the prevention of many diseases—such as scarlet fever, measles, small-pox—we have to face the problem of aërial disinfection, with all its difficulties. The only experiments made to test the effect of disinfectants, in the form of vapours, on a dried animal contagium, which I have seen detailed, are those on vaccine virus by Drs. Dougall and Baxter. The general result of these was to show that concentrated vapours destroyed the contagious quality of the virus when they operated for a sufficient length of time, just as the same agents in substance robbed fresh liquid vaccine of its power of communicating vaccinia. One other point is necessary again to adduce, and that is that the septic microzymes so abundantly found in ordinary processes of putrefaction are destroyed by the same agents used in nearly the same strength. These preliminary statements have now brought me to the ground and reason of my own experiments. Some of the animal contagia, as those of scarlet fever, measles, and some others, are almost unknown to us as objects of direct observation; but we have every reason to assume that they are subject to similar vital conditions with those which have been made the subjects of experiment, and therefore will have their virulence annulled by agents which act in that way, either on septic microzymes or on vaccine virus. My experiments have been made with these septic microzymes, which are always attainable, and whose death or continued existence can be proved with greater certainty than is possible in the case of the animal contagia by the method of inoculation, which is always liable to some fallacies. It is known that bacteria of different sorts, and especially these septic organisms, can live and multiply in a perfectly clear solution of certain saline matters, and the mixture known as Cohn's solution is admirably adapted for their cultivation. I used a slight modification of that originally recommended by Professor Cohn, composed of the following ingredients :—

Tartrate of ammonia	2
Sulphate of magnesia...	1
Acid phosphate of potash	1
Chloride of calcium	$\frac{1}{10}$
Distilled water	200

When this solution is boiled and preserved from any contamination it remains clear for an indefinite time; but if the smallest portion of any substance containing the septic organisms, called by botanists the *bacterium termo*, is added, it gradually becomes milky, the rapidity with which this occurs varying with the temperature at which the fluid is kept. The mode of procedure which I adopted was as follows:—I obtained a supply of the bacteria by adding a few crushed peas to warm water and leaving the mixture till it emitted a putrid smell, when it was found on microscopic examination to be swarming with these and other organisms. Then, to obtain them free from admixtures, I inoculated a portion of Cohn's solution with a minute drop of this putrid fluid, with the result that in less than two days the previously limpid solution had become quite opalescent. The bottle in which it was contained was shaken up, so as to obtain a uniform mixture, and a piece of filter-paper soaked with this, and then carefully dried in the sun for several hours. This bacterialised paper was preserved between the leaves of a book, and small portions of it used as required. To guard against fallacies I used the following precautions:—A number of small phials were taken, containing each about a fluid dram of Cohn's solution, and after being carefully plugged with baked cotton wadding, they were kept immersed in boiling water for a few minutes, so as to ensure the destruction of any bacteria which might by chance have obtained admission. After cooling, a portion of the bacterialised paper, which had been subjected to some disinfecting process, was put into one of them, the plug being removed for as short a time as possible. For the purpose of saving time a number of phials were thus charged and put aside in some protected place at the ordinary house temperature. As a check I placed beside them one phial to which nothing was added, and another into which a piece of the bacterialised paper, pure and simple, of the same size as the others, was put. If the phial containing only boiled Cohn's solution remained clear, this was a proof that there had been no accidental contamination, while if the one to which paper not disinfected had been put became opalescent, it was evident that the bacteria in it were alive (in the sense that a dried seed is alive) at the time the experiment was carried on. No experiment was held to be satisfactory unless both of these tests were fulfilled.

The endeavour was made to apply the disinfecting process in such a way as to allow of the results attained being made a guide in the practical use of these agents in every-day life; and in the use of vapours the time required for destroying the bacteria was the point investigated, the concentration being that which could be attained by the usual simple methods.

I.—Experiments with Carbolic Acid.

A wide-mouthed 8-oz. bottle was used, about a dram of crystallised carbolic acid being put into the bottom of it. The pieces of paper were suspended from a hook on the under side of the cork, which was fixed tightly in, and the whole left at the ordinary temperature of the atmosphere for carefully noted periods. A good deal of time was lost in feeling my way, in the absence of any knowledge at the time of similar observations.

(1.) Two pieces of the paper were exposed to the carbolic vapour for 9 hours and then introduced into the solution. In both cases opalescence began to appear in 42 hours, showing that the bacteria had not been destroyed; though as the phial into which undisinfected paper had been put began to be coloured in 36 hours, it appeared as if some of them had been killed, or at least in some way paralysed.

(2.) Two pieces exposed to vapour for 19 hours. Both remained clear.

(3.) One piece each 11 and 14 hours. Both remained clear.

Suspecting now that the air contained in the bottle had not had time to become saturated with the carbolic vapour in No. 1, which was begun as soon as the crystallised acid had been put into it, and in view of the positive effect in Nos. 2 and 3, I next tried some shorter periods.

(4.) One piece each exposed to the vapour for periods of $2\frac{3}{4}$, $3\frac{1}{2}$, 5, and 7 hours. The first two became opalescent, whilst the others remained quite clear. This experiment I considered quite conclusive, as the opalescence began to appear in the following order:—With the undisinfected paper in 60 hours, with that exposed for $2\frac{3}{4}$ hours in three days, and with that for 3½ hours in 4 days. The longer time required in all than in Exp. No. 1 was due to the different temperature of the atmosphere, the first having been carried on in hot weather, and this in cold.

It follows then that with the fullest possible concentration of the carbolic vapour at ordinary temperatures an exposure of more than 3½ hours is necessary to ensure the destruction of the bacteria. As the conditions, in ordinary measures for disinfecting the air of a room by means of carbolic acid, can scarcely be made so favourable as in a closely-corked bottle, it must be evident that, as generally used, carbolic acid is useless for the purpose. To bring out this satisfactorily, however, I performed the following supplementary experiments.

(5.) A tin of carbolic powder was taken, and all the perforations in the lid opened. The powder was then shaken up and two pieces of the paper left suspended close above it, one for 10 and the other for 24 hours. The solution into which they were put became opalescent with both in 3 days.

(6.) Two pieces were sprinkled freely over with the carbolic powder, and left uncovered for 10 and 24 hours respectively. With both the solution remained perfectly clear after 14 days. The powder was therefore good and showed itself useful when applied in substance, but the result of the whole series is to show that leaving vessels containing carbolic acid or this carbolic powder in a room is useless as a measure for destroying contagion, and may indirectly be harmful by giving a false sense of security, and thus preventing the use of more efficient measures.

II.—Experiments with Sulphur Fumes.

(1.) One piece each exposed for 5 and 15 minutes to the fumes of sulphurous acid obtained by throwing sulphur on hot charcoal. The paper was suspended from a wooden box inverted over the vessel containing the charcoal pan, which was placed at the opposite corner. The box was not very close, and the fumes escaped freely. The solution containing the piece exposed for 5 minutes became cloudy in 60 hours; that with the 15 minutes piece remained transparent.

(2.) Two pieces again in a closer box, but without very copious evolution of fumes, one for 5 the other for 10 minutes.

Both caused the solution to become milky, though earlier by 12 hours with that exposed for only 5 minutes.

(3.) Two pieces for 3 and 10 minutes in a close-fitting box, the vapour being more copiously evolved. The 3 minutes piece became opaque in 60 hours, whilst the 10 minutes one remained quite transparent.

It follows from the whole series that whilst it is possible to destroy the dried microzymes by an exposure to sulphur fumes for 10 minutes, it can only be done under very favourable conditions. An exposure for 15 minutes, if at all thorough, will usually be sufficient.

III.—Experiments with Ozonic Ether.

These were carried on in a bottle in the same manner as with carbolic acid, about half a dram of the ether being put into the bottom of a wide-mouthed bottle of about 5-oz. capacity, the pieces of paper being suspended from a hook on the under side of the cork.

(1.) One piece each exposed to the vapour of ozonic ether for 10, 30, and 60 minutes. The 10 minutes piece caused opalescence in 4½ days, the same time as the bacterialised paper. The other pieces left the solution unaffected.

(2.) One piece each for 10, 15, and 20 minutes. The 10 minutes piece caused only a slight opalescence after 5½ days, the other pieces remaining transparent.

It is clear from these experiments that in ozonic ether we have a powerful disinfecting agent, from 10 to 15 minutes of full exposure being sufficient to destroy the dried microzymes, and presumably the specific contagia of zymotic diseases. It is true that the high price of the ozonic ether would preclude its free use on ordinary occasions. These experiments are the only ones with which I am acquainted, as carried out in an exact scientific manner, and they have considerable interest in their bearing on the external application of ozonic ether in the form of ointment, as recommended by Dr. Day, of Geelong, for the purpose of destroying the contagium, and thus checking the spread of scarlet fever. It is very possible that direct contact with any contagious particles will render them powerless; but in view of the time required with the most concentrated vapour attainable, it is scarcely possible that the amount escaping into the air in the course of the process of inunction can have any effect on dried particles of contagium, which may chance to be floating about, or resting on walls or other surfaces. On a small scale, and where the conditions approximate those of the experiments just detailed, the ozonic ether may therefore be used with advantage.

IV.—Experiment with Chlorine.

The general impression in recent times is that chlorine does not deserve the great reputation it formerly enjoyed as a disinfectant, and, indeed, experiments have tended to show that when the gas is dry it has little or no power as a bleaching agent or as a parasiticide. I made one experiment in which the bacterialised paper was exposed, in a wooden box with a loosely-fitting lid, to the gas, evolved in the usual way by adding a few drops of muriatic acid to chloride of lime. The chloride of lime was rather damp, and a good deal of moisture was carried up with the gas. Three pieces of the paper were left suspended in the box for 1, 3, and 4½ hours respectively. The solution containing the 1 hour piece became milky in 4½ days, the other two remaining quite clear.

It appears then that, used in the manner described, chlorine, though not equal to sulphurous acid, is more powerful than carbolic acid. As ordinarily used, however, it can serve no good purpose, and sprinkling small quantities of chloride of lime on floors and other surfaces, in the hope of affecting any contagious matters floating in the air, must really be regarded as mere trifling.

V.—Experiments with Dry Heat of 212° Fahr.

These may not have very much value ; but as I have not met with similar ones, they may be given for what they are worth. In the absence of any more elaborate scientific armamentarium, I adopted the following procedure :—Two short, wide-mouthed bottles were carefully washed and then heated strongly in an oven, so as to ensure the removal of all moisture and the destruction of any organisms which they might by chance have contained. When still warm a piece of the bacterialised paper was put into each, and a good plug of baked cotton inserted into the mouth, which was further covered with a cap of the same material. They were then immersed in water, which was kept boiling for noted periods. The paper lying flat on the bottom of the bottles must have been exposed to a temperature nearly, if not quite, up to 212° Fahr.

(1.) One piece each for 10 and 30 minutes. The solution in both remained transparent, but I was somewhat doubtful of the trustworthiness of the result, as that which contained the unheated paper showed only a slight cloudiness after 4 days. This circumstance will be referred to again.

(2.) One piece each for 15 and 45 minutes. The solution with the 15 minutes piece became cloudy only in 4 days, the test bottle being opalescent at the end of 2½ days. The 45 minutes piece had no effect.

(3.) One piece each 15 and 25 minutes. Solution in both cases remained transparent after 12 days.

The conclusion come to, therefore, is that an exposure of these microzymes to a temperature of about 212° Fahr. must be continued for at least 15 or 20 minutes to ensure their destruction.

Two circumstances of considerable interest came out in the course of the investigation, which I have reserved for separate notice. The first was that when the bacterialised paper had been kept for between two and three months, the organisms seemed to have lost their power of reproduction. What the cause may have been I am not prepared to say, but that this happened was certain, and it caused a good deal of confusion and perplexity in my mind, till I suspected the state of matters and prepared a fresh stock, with which satisfactory results were at once obtained. The paper was kept between the leaves of a book, and was dry and exposed to very little rubbing. Could it have been that in course of time the desiccation of the bacteria became so complete as to be incompatible with continued vitality? Whatever the reason, it seems to follow that this particular species of bacterium cannot be kept in the dried state for very long periods without losing its vitality.

The other point is also, I think, of some interest, as showing the varying capacity of resistance offered to disinfecting processes by the germs of different low vegetable organisms. On a good many of the pieces of the paper which did not cause opalescence of the solution there appeared a copious growth of white mould, apparently the ordinary *penicillium*. The spores must have fallen on the paper at the time when it was exposed to the air, and they must have been subjected to the same destructive influences as the bacteria; and as they developed an abundant mycelial growth in several instances where the bacteria had undoubtedly been killed, it is evident that they possessed greater powers of resistance. In the detailed notes of my experiments I find that the mould appeared on paper which had been exposed to the vapour of carbolic acid for as long as 8 hours, a period of 3½ to 5 being sufficient for the destruction of the bacteria. On none of the pieces exposed to the fumes of burning sulphur was there any growth of mycelium. The ozonic vapour, again, though capable of destroying bacteria exposed to it for 10 or 15 minutes, apparently had not injured the spores of the fungus after 60 minutes. Again, whilst the chlorine killed the bacteria when applied for something over an hour, two pieces of paper, exposed to it for 3 and 4½ hours respectively, showed a copious growth of mould. Even to heat the *penicillium* spores showed greater power of resistance. Thus the mycelium appeared on each of the two pieces of paper which had been treated for 15 and 30 minutes respectively, the bacteria being killed in both instances. None appeared on the paper which had been treated for 45 minutes.

The conclusion to which I am brought, therefore, by the concurrent results of all these experiments is, that the spores of fungi are less easily destroyed than dried septic organisms, and presumably than dried contagium of zymotic diseases—as Dr. Baxter's experiments with dried vaccine showed its power of causing cow-pox to be destroyed by carbolic vapour in about 30 minutes, by sulphurous acid in 10 minutes, by chlorine in 30 minutes, and by a dry heat of 185° to 194° Fahr. for 26 minutes. He ventures to express the opinion—founded not on his own experiments, but on a few made by others on yeast and *penicillium*—that the influence of disinfectants on such fungoid spores affords no measure of their action on contagia, since the former are very much more susceptible to adverse influences than the latter. This opinion is directly contradicted by the results of the exact experiments here detailed, which show that any disinfectant which destroys *penicillium* spores in the dry state may be depended on to destroy bacteria, and so, presumably, contagia, which are even more easily destroyed, as a comparison of my observations with Dr. Baxter's on vaccine clearly shows.